further than the expression of certain facts of general observation, which in themselves constitute no proof. Schwendener's theory of the influence of mechanical contact has long held the field, in spite of the fact that no such contact can be invariably proved to exist, or even to exert any mechanical action; so much so, in fact, that it has been regarded as possible to dilute the theory to one of vague "stimulation." Dr. Iterson, by following along the well-worn paths of previous observers, has reached very similar conclusions; and it must be admitted that continual study of the best exhibitions of the uniform construction of vegetative shoot-systems naturally impels the observer to the old and familiar view of Hofmeister, that appendages cannot help themselves, but arise in the next "widest gap" between pre-existing ones, as they are seen to do; such a statement represents no solution of the problem, but is, in fact, a confession of failure.

On the other hand, by approaching the subject from the standpoint of floral ontogeny, in which the most complex phyllotaxis-systems can be observed developing before one's eyes according to a perfectly defined architectural plan, in which the relation of the individual members may, however, be practically anything whatever, wide gaps being left in some places, spirals mixed with circular construction, and members apparently "omitted," as well as appearing "out of their turn," an investigator equally inclines with Schumann to the view that contact-relations present no contributory cause whatever to the phenomena of the initial phases, which can only be referred to autonomous growth-impulses within the substance of the shoot-apex; a region which, consisting as it does of undifferentiated cell-units, is beyond further possibility of observation. Among the general conclusions for straightforward "constant-phyllotaxis," Iterson reiterates the stock considerations of "bulk-ratio," "contact-relations," and the principle of the "widest gap" (p. 291).

The fallacy of the widest gap has been exposed over and over again; it is sufficiently obvious to the unprejudiced eye in the appearances presented at the apex of the common Fern, or shoots of Water-lilies; primordia do not invariably arise in close contact with each other, but may be widely spaced out at first. The same want of contact, or any connecting sequence between one series of members and the next, is a common phenomenon in floral ontogeny which also includes cases of such absolute irregularity that the necessity for a "mechanical law" for their production becomes an absurdity; while in examples of perfect regularity of construction, the case of extreme mathematical interest centres in those few instances in which successive whorls do not fall into the gaps of their predecessors at all, but are accurately superposed; such cases occur in certain living species of Mesembryanthemum, though the significance of this formation in the case of fossil plants may still be open to question. It is thus a matter for regret that Iterson should revive the conceptions of "close-contact" and "widest-gaps," which have seen so much service in the past; while again the conception of "bulk-ratio," or the relative size of the primordium with regard to the axis on which it is "inserted," though extremely useful in dealing with the difficulty of distinguishing between systems which involve numerals of the same summation series, *e.g.* 3 : 5 : 8, &c., can yield no practical solution of the difference, for example, between a 3 : 5 construction and a 3 : 4, or, again, of the essential difference between spiral and circular arrangement. It is also sufficiently obvious that the causes which determine the relative rates of growth (which lie behind the relative size) must have existed in the actual substance of the growing-point some time before the primordia became visible to the eye as a definite outgrowth, and, admitting the absence of any necessity for close-contact, the spacing of the new centres of growth is presumably more important than their actual size; once the centres are initiated, the new growth-impulses from them are continued until they ultimately make lateral contacts as a wholly secondary phenomenon.

Dr. Iterson's volume affords an admirable introduction to the subject, and most of the branches are indicated; considerable value attaches to the *résumé* of the theories of Schwendener and Celakovsky, copiously illustrated with excellent figures (more than 100 text-illustrations, and 16 plates); these bear sufficient witness to the enthusiasm of the author in this most absorbing field of speculation, though when all is said we appear to be no nearer the solution of the problem than ever; it only gains in complexity where it seems to be most regular and simple; since an absolutely irregular construction can clearly have no explanation at all, it simply grows as one sees it grow, and can neither be accurately described nor imitated. To those who seek for the inner and ultimate cause of the phenomena, the subject still presents an indefinite field of research.

Few botanists appear to realise the extent to which a proper appreciation of the subject of phyllotaxis is involved in the morphological consideration of plant-growth, and floral construction and even phylogeny; the marvellous standpoint that a dimerous flower is simpler, and therefore more primitive, than a trimerous one, and a trimerous than a pentamerous, constitutes one of the vitiating factors of the systematic work of Eichler, and is still reflected in the modern German school of classification.

ORGANIC CHEMISTRY FOR MEDICAL STUDENTS.

Text-book of Organic Chemistry for Medical Students. By Dr. G. v. Bunge. Translated with additions by Dr. R. H. A. Plimmer. Pp. ix+260. (London: Longmans, Green and Co., 1907.) Price 6*s.* net.

THE rapid advance which has marked the progress of physiological chemistry in the last twenty years, owing mainly to the remarkable researches of Prof. Emil Fischer, has emphasised the necessity of a sound knowledge of organic chemistry for all students of medicine. But organic chemistry has undergone developments in many other directions, and new compounds have multiplied at a most bewildering rate. It is clearly undesirable for the student of medicine to become acquainted with any

large proportion of the hundred thousand compounds which organic chemistry is said to include. He might, after a firm foundation had been laid, study with advantage only those special subjects which come within his sphere of interest. He cannot very well know how to select these for himself, and Prof. Bunge has therefore attempted to do it for him.

On the whole, Prof. Bunge has been very successful in the choice and arrangement of his materials, and has produced an eminently readable book. But the task cannot have been an easy one. In a small volume of 250 pages, which is assumed to start with the rudiments and finishes with such complex vital products as the purines, the proteins, and the alkaloids, there is a danger that the treatment may be diffuse and superficial. But though this is certainly not the case, it must be confessed that some preliminary knowledge of analysis, molecular-weight determinations, and especially about methods of studying structure, is desirable, if not indeed necessary, if the subject is to be understood. In support of this it may be pointed out that the structural formula of oxalic acid is given on p. 2, of glyceric aldehyde and dioxyacetone on p. 5, and of hippuric acid on p. 8, without any previous reference to Kekulé's structural laws. But this appears to be the only serious defect, and one which the student can easily remedy by a little preliminary study.

The chapters are written in a manner well calculated to stimulate the reader; indeed, no organic text-book within the writer's knowledge is so full of human interest. The following few errors have been noted:—Chlorine does not convert aldehyde into chloral, but mainly into butyl chloral (p. 50). It is not true that "no one has yet succeeded in obtaining directly by synthesis either a *d*- or a *l*-compound" (p. 79). On the contrary, *asymmetric synthesis* is an accomplished fact. A racemic compound and a mixture of enantiomorphs are not synonymous, and the difference is indicated by *r* and *dl* (p. 89). The author refers to the separation of synthetic tartaric acid into its *d*- and *l*-components by Jungfleisch as causing a great sensation, "for up to that time many chemists thought that optically active compounds could only be formed by the living cell" (p. 89). There must surely be some confusion here, for did not Pasteur resolve racemic acid? Pasteur, it is true, considered asymmetric synthesis, or the formation of one enantiomorph without the other, as a peculiar property of living matter, but that is another thing altogether. Finally, on p. 147 occurs the old story of Wöhler's discovery of artificial urea in 1828, a date which tradition and the text-books have fixed upon as that of a two-fold event—the first organic synthesis and the downfall of the vital-force theory. In reality it was neither the one nor the other, and perhaps the following observations may help to put the matter in a clear light.

The preparation of natural products in the laboratory began before Wöhler was born, for in 1776 Scheele obtained oxalic acid by oxidising sugar. Doebereiner's preparation of formic acid from tartaric acid in 1822, and Hennel's synthesis of alcohol from olefiant gas were both prior to Wöhler's discovery. That Doebereiner's discovery received contemporary recognition is evident from Berzelius's reference to it in the *Jahresbericht* for 1823. "Doebereiner," he says, "has made the remarkable discovery that formic acid may be produced artificially." Now Liebig, in his treatise of 1840, falls into a curious error, which may lie at the bottom of the text-book myth. In reference to formic acid he writes, "Doebereiner was the first who prepared it by chemical means," whilst in another place he says, "Woehler found a way of obtaining urea artificially, and it was the first substance formed in the animal-life process which had been successfully reproduced by chemical means." Now formic acid is as much a product of the animal-life process as urea, and no real distinction can be drawn between them.

It is clear, therefore, on Liebig's own showing, that of the two artificial products, Doebereiner's has the prior claim. How little Wöhler's discovery served to remove the belief in a vital force is very clearly indicated in Gerhardt's "Précis de Chimie Organique," published in 1844.

"A number of animal and vegetable substances have been reproduced by acting with oxygenating agents on more highly carbonised compounds . . . thus, the chemist has followed a path entirely opposed to that pursued by vegetable life . . . one need not therefore feel astonished that he has not yet produced cerebral matter, nor the constituents of the blood, nor equally complex substances."

Thus the vital-force theory did not suddenly collapse, as generally stated; on the contrary, it died a slow and lingering death. We may, indeed, ask, is it quite dead yet? For to quote the words of an authoritative contemporary writer, "the testimony of pure chemistry cannot as it at present stands be legitimately interpreted into a direct negation of vitalism in any form."

There only remains to add a reference to the work of the translator. Dr. Plimmer has not only rendered the German into excellent English, but has added very considerably to the text. J. B. C.

OUR BOOK SHELF.

(1) *Some Nature Biographies: Plant, Insect, Marine, Mineral.* By J. J. Ward. Pp. xvii+307; illustrated. (London: John Lane, 1908.) Price 5*s.* net.

(2) *The Fairyland of Living Things.* By R. Kearton. Pp. viii+182; illustrated. (London: Cassell and Co., Ltd., 1907.) Price 3*s.* 6*d.*

(1) MR. WARD'S little work, which consists of a series of articles originally published in the *Strand, Pall Mall, English Illustrated,* and other magazines and periodicals, may be regarded as a kind of kinematograph in book form, and may be unreservedly commended to all nature-lovers. One great feature of the several life-histories is that they are in the main based on actual personal observation, and that, too, of a kind which demands constant attention and the expenditure of no inconsiderable amount of time. In his preface the author very modestly suggests that he is entitled to the credit of being the pioneer in certain forms of insect photography, and to this credit, so far as our information goes, he is fully entitled. Nothing in nature-photography can, indeed, be more interesting than his pictures of the sequence of events which herald the complete liberation of the butterfly or the moth from its chrysalis,